Response to Comments on the Revised Draft

Allen Harbor Landfill and Calf Pasture Point Marine Ecological Risk Assessment Report

Naval Construction Battalion Center Davisville, Rhode Island

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RESPONSE TO COMMENTS FROM EPA ON REVISED DRAFT, ALLEN HARBOR LANDFILL AND CALF PASTURE POINT MARINE ECOLOGICAL RISK ASSESSMENT REPORT

General Comment #1
Most of the overall data interpretations and conclusions of the report are reasonable, sound, and supported by the weight of evidence presented, which reveals clear ecological risks and apparent impacts in the saltmarsh and nearshore, intertidal mud flat habitats of Allen Harbor proximal to the landfill.

Response
No response required.

General Comment #2
Unfortunately, as discussed below in more detail, some of the data analyses provided in support of these conclusions are incomplete, inappropriate, and/or as presented, of little practical value to risk managers in quantifying the actual magnitude of risks from specific contaminants of concern (COCs) or classes of COCs and/or in concisely and clearly illustrating what the key COC drivers of average and maximum risks are within the salt marsh, intertidal mud flat, and subtidal exposure zones.

Response
Comments and comment responses below will elucidate issue areas.

General Comment #3
The primary problem with the technical approach and data presentation in the report is a conceptual one, which has very significant "ripple effects" on the content and discussions of the entire document. The overly broad, misleading, and incorrect definition of the term hazard quotient (HQ), as including not only criteria-based risk quotients but also media-specific concentration ratios of Allen Harbor habitats versus reference habitats, is a fundamental flaw in the ERA methodology that confounds most of the risk discussions throughout the report. These ratios of COC levels in sediments of one location versus another location are not HQs, they are merely concentration ratios and such should be renamed, "Concentration Ratios (CR)" for clarity. Toxicity benchmark criteria must be used to calculate true and meaningful HQs, both for the Allen Harbor and reference locations.

Response:
Comparisons of site and reference location chemistry data do not conflict with the risk assessment paradigm as assessment of incremental risks at the site is a necessary component for the ERA. The comparison of site and reference COC concentrations is discussed in the approved work plan. The Navy concurs that a better clarification and separation of discussions of baseline risks (i.e. literature effects values vs. site concentrations) and incremental risks (i.e. reference location data vs. site concentrations) is needed and will be provided as presented below. The

Navy acknowledges that the terminology used to discuss incremental risks, e.g. "reference-based Hazard Quotients" has led to confusion and conflict with "criteria-based Hazard Quotients". In the revised report, the terminology "Hazard Quotient" will be reserved only for comparisons of site concentrations against criteria-based benchmarks. The previously used "reference-based HQ" will be changed to "Incremental Risk Quotient" to eliminate this confusion.

The following text will be added as introductory material to Section 6.0 to clarify the necessity of Reference-based Hazard Quotients for the Risk Assessment.

> "A key objective in the Allen Harbor Marine ERA is the assessment of incremental, site-related risks to Allen Harbor biota. Here, "incremental" means those impacts which are above and beyond that which would be expected to occur at the site in the absence of Navy-related activities. Thus, there are two central questions which must be addressed to establish incremental risk:
>
> - What is the baseline, absolute risk to aquatic receptors at the site caused by COC exposure?
>
> - What is the added risk caused by Navy-related activities at the site caused by enrichment of COCs beyond that expected from regional sources?"
>
> "In this ERA, a common approach is taken in approaching both data needs involves the use of the Quotient Method for Ecological Risk Assessment (Suter, 1990). The method involves the comparison of a measurement or effects endpoint to a predicted or measured environmental concentration. A measurement endpoint as defined by Suter (1990) is "an expression or an observed response to a hazard". A common expression of hazard is the degree to which COC concentrations exceed toxicological benchmark (criteria) values, and the inferences to be drawn from this calculation are as follows:
>
> 1). Hazard Quotient (HQ) = Site COC Concentration/Effects Concentration
>
> - COCs with HQs < 1.0 are not key baseline risk drivers;
>
> - The probability of specific COCs being key baseline risk drivers increases with the HQ value.
>
> These criteria-based HQs directly address the baseline, absolute risk to aquatic receptors at the site caused by COC exposure. This calculation alone, however, does not address incremental risks. Risks at the site can be related to risks at reference locations as follows:
>
> 2) Incremental Risk Quotient (IRQ) = $HQ_{site}/HQ_{reference}$
>
> where the resultant value will indicate whether baseline risks at the site are greater or less

than that occurring at the reference location. Following the interpretation of HQ data described above, the inferences to be drawn from this calculation are as follows:

- o COCs with IRQs < 1.0 are not key incremental risk drivers;
- o The probability of specific COCs being key incremental risk drivers increases with the IRQ value.

"It is notable that for the IRQ calculation, the COC-specific Effects Concentration is common to both denominators of the site and reference HQ values, hence the functional form of Eq. 2 reduces to the ratio of the site COC concentration to the respective reference COC concentration. This fact allows the IRQ calculation to be applied to those COCs for which effects criteria have not yet been developed, since the IRQ value is mathematically independent of the effect concentration. However, because recent trends in the development of sediment criteria for organic contaminants is to express the threshold effects concentration as normalized to the organic carbon content of the sediment, the IRQ evaluation must include both raw and TOC-normalized COC concentration ratios".

"In the following sections, the discussion of incremental exposure risks (Section 6.1) is presented first so as to provide better focus to subsequent discussion on which COCs are the key risk drivers. Risk drivers are evaluated from field effects data in Section 6.2, transport pathways linking incremental exposure risks with COC bioaccumulation and toxicity are discussed in Section 6.3 and 6.4, and the evaluation of observed effects relative to known toxicological benchmarks through Hazard Quotient calculations in Section 6.5. The risk evaluation is extended to consideration of baseline and incremental risks as suggested from COC concentrations in the tissues of target receptors (Section 6.6). Finally, and analysis of uncertainties associated with the above interpretations is provided (Section 6.7)"

Suter, G.W. 1990. Endpoints for Regional Ecological Risk Assessment. Environ. Management 14:9-23.

General Comment #4
This misuse of the risk quotient method also compromises the calculations (and practical value) of the aggregate, COC class-level, hazard indices (HIs), that represent the sum of HQs for each location and/or the sum of the mean HQs among all samples within a particular exposure zone. Only after these true HQs/HIs are systematically and separately generated can the incremental Allen Harbor risks above those found at background locations be quantified. These point-by-point calculations of Allen Harbor:reference sample COC concentration ratios also leads to a risk analysis of little practical value because it compares contamination levels rather than the actual organismal risks from these levels. Only the true, criteria-based HQs/HIs should be presented as indicators of ecological risk, since they serve both to document key risk drivers

and offer the best hope of clarifying the chemical causes of observed toxicity (amphipod and sea urchin) and/or biological condition indices suggesting adverse effects.

Response:
The Navy does not concur that the COC class level indices have been compromised as the two approaches were not intermixed; Figures 7.2-2 and 7.2-3 include only Hazard Quotients and Indices based on published literature value comparisons, whereas Figures 7.2-4 to 7.2-6 include only Incremental Risk Quotients (formerly Reference-based HQs) and Indices based on reference location comparisons. Only in the final risk summary were results of the two approaches combined, and here, a revised table will be prepared to separate baseline from incremental risk weights of evidence.

The Navy concurs that the "criteria-based Hazard Quotient" as reported in the ERA is an accepted expression of the degree of *true, baseline* risk. The Navy does not, however, concur that only the true, criteria-based HQs/HIs should be presented as indicators of ecological risk, because this particular weight of evidence does not address *incremental* risks, nor is it a substitute for the presumption of risk using the weight of evidence summary as presented in the ERA. The Navy also feels that the incremental risk calculations, as discussed in the Response to General Comment #3, above, are of practical value for documenting key risk drivers. On a qualitative basis, they assist in clarifying the chemical causes of observed toxicity and/or biological condition because it is unlikely that those COCs posing less risk at the site than the reference location (e.g . IRQ < 1) are key risk drivers. It is also reasonable to postulate that the extent of adverse effects at the site may be also (perhaps more) related to the degree to which the exposure exceeds background conditions, since organisms may be acclimated to COC concentrations on a regional scale, but are "unprepared" for dramatically elevated exposures.

General Comment #5
The concept of "tissue-based HQs," as presented in the report, is similarly flawed, in that a true tissue-based HQ would consist of the ratio of the tissue concentration for a COC to the known toxicity threshold for a particular body burden (concentration) for the organism that accumulated the COC. Although this offers insight as to "how much worse" COC bioaccumulation seems to be in Allen Harbor than in the reference locations, this question is not pertinent to the primary objectives of a Comprehensive Environmental Response, Compensation, and Liability Act of 1980 (CERCLA) ERA as it does not help risk managers to assess for which COCs incremental risks are unacceptable and/or the spatial extent of cleanup warranted for such COCs within Allen Harbor.

Response
Following the logic presented in Response to Comment #3, tissue-based HQs provide an estimate of incremental risk due to tissue residues; even if tissue-based effects thresholds were used in the calculation, the incremental risk calculation presented in the ERA would remain unchanged. Elucidation of extent of incremental risks is a precursor to determination of unacceptable baseline risks; there should be no expectation that a particular COC concentration

in a target species which is greater at the reference location than at the site should be considered a key risk driver. These incremental risk calculations should be useful to risk managers; it would be unreasonable to set clean-up goals that require lower COC concentrations at the site than the reference location, since the objective could not be met over the long term.

The Navy does concur, however, that further evaluation of baseline risk of tissue residues be evaluated. Unfortunately, ecological effects threshold criteria for unacceptable tissue residues are not generally available, such that the second step of the evaluation, being absolute assessment of risk from COCs showing incremental risk, is problematic. One potentially promising approach, which will be investigated further for the revised document, is the examination of narcosis as a mode of toxic action via comparison of summed molar concentrations of class-specific COCs with threshold concentrations for a narcotic effect (e.g. McKay, et al. 1992). This approach has received additional validation and refinement in recent studies (DiToro and Maiello 1995; Shepard, 1995).

McCarty, L.S. and D. McKay, 1993. Enhancing Ecotoxicological Modeling and Assessment. Environ. Sci. Technol. 27(9):1719-1728.

DiToro, D.M. and J.A. Maiello, 1995. A proposed sediment quality criteria for narcotic chemicals. 2nd SETAC World Congress, Vancouver, CA Nov. 6-9, p. 2.

Shepard, B.K., 1995. Tissue screening concentration values for assessing ecological risks of chemical residues to aquatic biota. 2nd SETAC World Congress, Vancouver, CA Nov. 6-9, p. 167.

General Comment #6
Navigating through the large volumes of raw data tables and graphs presented in the main report and its appendices is very difficult. These raw data could and should be reduced for a more concise and meaningful presentation using one or more, "roll-up" matrices to report average, medium-specific, criteria-based HQs and COC-class level HIs for each exposure zone and for the reference stations. The resulting roll-up tables would be very useful for risk managers by presenting much needed concise snap-shot look at the risks at the Harbor. This type of roll-up was presented for qualitative results in Table 7.3-1, such as biological effects and body burden data. Although it nicely summarizes the qualitative weight of evidence, this table suffers from the qualitative inferences as to COC-class level, incremental, site-derived exposure risks that are not systematically based on ecotoxicological risk quotients. Rather, these chemical "exposure" entries are inappropriately based on an "apples and oranges" combination of both true, criteria-based risk quotients (HIs) and Allen Harbor:reference habitat COC concentration ratios for physical media, that are misnamed as "HQs" and "HIs".

Response
Summary, roll-up tables will be brought forward from the Appendices, including separate presentation of Site and Reference data, to report average, medium-specific, criteria-based HQs

and COC-class level HIs for each exposure zone and for the reference stations. The summary table will be revised to separate baseline from incremental risks as defined in Response to Comment #3. above.

General Comment #7
The net effect of using this "hybrid" risk quotient method, coupled with the current presentation of volumes of raw data, is a non-reader-friendly document that is often very confusing, even to perennial technical reviewers of this project. As a result, the report also fails to clearly and concisely quantify the incremental, site-associated marine ecological risks within the context of systematically quantified, criteria-based, background risks at the two reference locations.

The Navy feels that the presentation of station-specific results within the main report is a necessary step to justify the zonal segregation of Allen Harbor environments. Data were presented graphically rather than in tabular format to give a "snap-shot" overview of the results. More simplistic data presentations could have been performed, (e.g. summary statistics by broad habitat type) but these approaches would not provide the necessary data to delineate those areas where remediation should be considered. At EPA's request, roll-up tables by broad habitat type (intertidal, vegetated wetland and subtidal) will be prepared to assess risk to the more mobile (especially terrestrial) species which might utilize all of Allen Harbor habitats.

only in graphical, all in tables

During the preparation of the report, significant effort was made to minimize and simplify the presentation of raw data (In fact, there was concern that the data presented was over-simplified). For example, organics data for PCBs and PAHs were simplified by presentation of data based on total concentrations only. Only one pesticide was presented. The metals were presented individually as no representative metal could be identified. The VOA data were also treated separately because of the desire to relate spatial distributions in groundwater with sediment concentrations.

However, the Navy concurs that raw data presentation should be further minimized where possible; for example, Incremental Risk Quotient (formerly "Reference-based HQ") presentations will be simplified to include only comparisons against average reference concentrations, hence eliminating separate presentations for each reference location. The Navy requests further input as to how raw data presentation can be minimized.

General Comment #8
The report provides incomplete calculations of criteria-based HQs/HIs, in part due to omissions of such HQ/HI tables that were presented for polychlorinated biphenyls (PCBs) and pesticides in the May 1995 Draft version (e.g., Tables A-1.1 and A-1.2) and the lack of such tables for organic COCs in surface water and in sediment pore water, the latter of which were to have been inferred via equilibrium partitioning (EqP), as requested by EPA in Comment No. 9 in EPA's letter dated June 30, 1995.

Response

The data omission of criteria-based HQs/HIs for polychlorinated biphenyls (PCBs) were in error and will be corrected. HQ/HI and incremental risk quotient tables for surface water organic concentrations will be developed for the revised report. The Navy concurs that a more extensive evaluation of risk from porewater organics is necessary and proposes a strategy as described in the following text of the report which will be added to Section 6.5.2 of the report to incorporate COC screening of porewater COC concentrations:

> "The calculation of organic COCs in sediment pore water via equilibrium partitioning (EqP) and subsequent comparison against appropriate effects-based benchmarks (e.g. Water Quality Criteria-Saltwater Chronic (WQC-SC) values) is limited; there presently exists only three PAH and four VOC benchmarks (see table 3.3-2 of subject report). To provide a more comprehensive screening using this approach, water-based COCs are proposed for calculation following the decision tree presented in Figure 6.5-1. This approach allows for calculation of "WQC-SC equivalent" benchmarks from Table 3.3-2, and assigns a degree of uncertainty to porewater Hazard Quotients resulting from use of the data".
>
> "A no uncertainty baseline (weight = 0) is applied to HQs derived from porewater concentrations normalized to WQC-SC values. For COCs possessing saltwater acute values, a 1:8 acute:chronic ratio is applied to derive the equivalent SC value with uncertainty weight of 1. Freshwater chronic data are used directly as screening values, but with relatively greater uncertainty (weight = 2). As with SA values, freshwater acute (FA) values can be converted using a 1:8 acute:chronic ratio, but with increasing uncertainty (weight = 3). Finally, sediment criteria (NOAA ER-M or if unavailable, Apparent Effects Threshold) values are used assuming 1% TOC for all stations (weight = 4)."

A draft of Figure 6.5-1 is included for EPA's review. The Navy requests EPA's approval for this screening approach.

General Comment #9

Tables of sediment chemistry data and "hybrid" HQs/HIs presented throughout Appendices A1 and A2, moreover, inappropriately averaged together both the COC concentrations and HQs/HIs for Allen Harbor with those of the reference habitats, thus defeating the main purpose of the reference habitat sampling. That purpose was to present average, criteria-based HQs/HIs separately for Allen Harbor versus reference habitats and thus to calculate NCBC site-related risk increments within Allen Harbor. These tables are also very confusing because they present meaningless data manipulations, such as the sum of point-specific HQs for each congener (in the 3rd from last column in each table) and also sum the point-specific congener sums, collectively, across all sample locations (at the bottom of this same column). The congeners should be (a) added only down the point-specific columns; (b) each sum multiplied by a factor of 2 to estimate Total PCBs; and then © these column-specific Total PCB concentrations should be averaged across columns, but separately within each of the discrete exposure zones (salt marsh [V#s], intertidal mud flats [W#s], and subtidal zones [D#s].) The resultant range exhibited among these

sample/column-specific Total PCB values then should be carried, along with their corresponding zone-specific mean concentrations, into a roll-up summary table of total PCB concentrations reported by individual zones. Since the same approach is needed for data reduction for other COC classes, all of these contamination and risk tables must be revised in the final report.

Response

Agree. Appendix tables will be revised as recommended by EPA. Summary statistics will be added to the appendix table to include that information which will be brought forward to the main document as summary roll-up tables. Habitat and zone-specific tables will be prepared separately for raw data, HQ/HI and IRQ data for each media (sediment, surface water, pore water, tissue).

General Comment #10

Attempts to identify the COCs likely to be responsible for biological effects, as indicated by observed toxicity in the bioassays, neoplasia incidence, and/or shellfish/fish condition indices, by spatially comparing COC concentrations with effects data, were not successful. This is not surprising since the approach to this analysis requested by EPA in Comment No. 9 on June 30, 1995, consisting of correlation studies of these effects data as a function of criteria-based HQs/HIs, was not followed systematically. It is significant that, where this approach was used to calculate ambient water quality criteria (AWQC)-based metal HQs for sediment pore water, more light was shed on the possible COCs causing observed toxicity. In order to further clarify which COCs may be causing the observed ecological effects of sediment contamination, this previously requested approach should be applied to all COCs in whole sediment and pore water.

Response

The Navy will prepare additional correlation analyses to systematically evaluate potential relationships between COCs and observed effects. The output will be matrix tables of r-squared values; those values with regression slopes greater than zero will be bolded. These data will be added as appendices. A roll-up table will be prepared for appropriate report sections to summarize COCs with R-square values greater than 0.5 and different from zero. Text will be added to the report to include interpretation of these data with regard to particular COCs that may be key risk drivers.

General Comment #11

Estimates of total PCBs are inconsistently presented in both the contamination and risk quotient tables, either incorrectly as the sum of congeners (e.g., Tables A.1-1.1 and A.1-2.1) or correctly as two-times (2x) the sum of congeners (Table A.1-3.1). Although the congener-based total PCB estimates had been correctly presented in Tables A-1.1 and A-1.2 of the May 1995 draft of this report, that version had incorrectly calculated (and thus underestimated by a factor of two) the criteria-based HQs/HIs for total PCBs, by dividing the sum of congeners by the Effects Range - Low (ER-L) value of 22.7 ppb. All tables in the final report must calculate total PCB levels (and associated HQs) using the factor of 2x to convert the sum of congeners into a total PCB estimate.

Response.
The report will incorporate the recommended changes.

General Comment #12
Despite the report's demonstration of acceptable comparability between older and newer data from successive phases of the Allen Harbor investigation, data from the Risk Assessment Pilot Study (RAPS; Munns et al., 1991, 1993) appear not to have been integrated statistically into the risk quotient calculations, as previously requested by EPA. Rather, these RAPS data were reproduced in the report (see Tables 3.1-1 through 3.1-4) and discussed only from a historical perspective. There is no explanation otherwise in the report as to whether and/or where/how these older RAPS and newer Phase III data for physical media and biota in Allen Harbor were merged/reduced to produce cumulative data summaries. This is a significant omission, because most of the RAPS sediment samples were from different subtidal locations than those sampled in Phase III. Also, in many cases the RAPS concentration data differ significantly from those for Phase III subtidal samples (e.g., maximum, subtidal Total PCBs reported as 103 ppb at AHD8 in Phase III [Table A1-1.1] versus a maximum, subtidal Total PCB level of 505 ppb reported at AH08 [different location] by Munns et al. [1991] and reproduced in Table 3.1-1 of the current report.) These RAPS data must be statistically integrated with Phase III data to revise both the contamination data summaries and ecological risk quotient calculations of the Final ERA Report.

Response
Results of RAPS studies were not statistically integrated into the Risk Quotient calculations. A variety of problems with incorporation of the RAPS data exist including that there are not identical analyte lists between studies, and in many instances, there are significant data gaps (e.g. missing analytes at various stations). These problems greatly complicate between station comparisons of data, particularly the Hazard Indices, since identical data lists are not available for summation. Despite these difficulties, the Navy agrees to provide a well documented, best effort to incorporate the data from Allen Harbor stations shown in Figure 3.1-1. Note that "seep" stations are not included in this analysis. These stations are on the landfill face above high tide and do not share the same habitat as intertidal target receptors; the samples were collected as source characterization purposes and not to characterize exposure.

General Comment #13
The purposes of the new, principal components analysis added to the report are not explained clearly. The methodological rationale for and process used to generate these statistical analyses, as well as how the results pertain to questions of ecological risk, should be clarified. This section may be more appropriately included in the fate and transport discussions of the Remedial Investigation Report.

Response
The following additional text will be added to the beginning of Section 4.2.6 to clarify the purpose of the principal components analysis to the report:

"Principal Components Analyses (PCA) were conducted on the data in attempt to better characterize differences in the source and pathways of COC transport to nearshore environments of Allen Harbor which would assist in explaining differences in patterns of bioaccumulation and toxicity to be discussed in Section 5. The approach taken in this analysis is to use the pattern of chemical mixtures at individual stations to identify station differences as to both the strength (concentration) and uniqueness of chemical constituents."

"A principal component is a numerical value which can be viewed as an aggregate variable expressing the degree of similarity for a particular feature of the data set. By design, every principal component is independent of one another, and hence expresses unique properties of the data set. Because differences in chemical concentration are one of the most unique features of the data set, the first principal component is primarily an indicator of COC concentration. Subsequent principal components express additional aspects reflecting nature of chemical mixtures. For stations which exhibit unique chemical patterns, the underlying compound-specific chemical data (in PCA units, called "loadings") are examined to determine those chemicals which drive unique chemical distributions at the station and thus the COCs which should be considered among the potential COC risk drivers for the risk analysis."

"Principal components analyses were performed using a conventional software package (Systat, 1995). In the present analysis, class-specific data sets (PAHs, PCBs and pesticides) were prepared as input data files to the software package as raw concentration data (z-score normalization was evaluated but rejected as the technique did not improve component score separation). The number of principal components generated by the package was limited to three, which was found to cumulatively express > 90% of the variation in the data."

The Navy's preference is to keep this section as part of the marine ERA. However, a summary of the results will be included in the RI report as it pertains to surface water and ground water transport.

General Comment #14
As reflected in some of the foregoing general comments on methodology and data presentation, and further discussed in many of the specific comments that follow, many of EPA's previous comments/requests have not been adequately addressed or satisfied in the current report.

Response
Foregoing comments addressed above; specific comments addressed below.

Specific Comment #1
Titles of tables and figures that do not present true, criteria-based HQs are very misleading and should be revised to accurately indicate that Allen Harbor:reference site concentration ratios,

not risk quotients, are being presented (e.g., Tables 6.3-1 through 6.3-4 and 6.6-1 through 6.6-4; Figures 6.1-1 through 6.1-16). (Although most of these figure titles in effect do show the ratios of criteria-based HQs, their titles are still misleading since criteria-based HQs were not first calculated for all of these sample locations.)

Response:
The Navy concurs that titles of tables and figures which present incremental risk based on reference location data may be confused with those presenting true, criteria-based HQs because of the use of Hazard Quotient terminology in both cases. The Navy will revise the terminology in figure and table captions as well as associated text for incremental risk presentations to indicate that incremental risk quotients are being presented; e.g. Tables 6.3-1 through 6.3-4 and 6.6-1 through 6.6-4; Figures 6.1-1 through 6.1-16 will begin as "Incremental Risk Quotients for .... ". Corresponding introductory references to Hazard Quotients will be changed to "Allen Harbor:reference site concentration ratios indicative of incremental risk" and will be abbreviated as IRQ in subsequent text for the section.

Specific Comment #2
Summary tables of (incorrectly calculated) criteria-based (ER-L) HQs, that had been presented in Tables A-1.1 and A-1.2 of the May 1995 draft, were omitted from this revised version of the report.

Response
The tables will be included in the draft final document.

Specific Comment #3
Figures 7.2-2 through 7.2-4 were missing from the review copy of the report, however they have been recently received.

Response
The Navy again apologizes for the inconvenience.

Specific Comment #4
Graphs of the biota-to-sediment bioaccumulation factors (BSAFs) and surface sediment metals data, respectively presented as Figures 6.3-1 through 6.3-4 and Table 5.3-2 in the May 1995 draft report, were omitted from the revised draft and should be restored. The metals data are essential and the BSAF figures were especially informative, particularly with respect to inter-species differences in BSAFs among shellfish, that should be accounted for in food chain models to be used in the terrestrial ERA (e.g., oysters and ribbed mussels have higher BSAFs than the soft and hard shelled clams; since these former two species are most accessible to terrestrial shellfish eaters, only they and their BSAFs should be used in the NCBC terrestrial food chain models.)

Response:

Graphs of Biota-Sediment Accumulation Factors (BSAFs), presented in the May, 1995 report were deleted in the revised draft because it provided a misleading impression that there exists inter-species differences in BSAFs among shellfish. For this reason, the Navy does not intend to restore these figures. Rather, results of the food chain analyses (Section 6.3) clearly show that there are no statistically significant differences in BSAFs among fish and shellfish species for non-ionic organic contaminants. This finding is consistent with EPA's equilibrium partitioning theory which states that bioavailability under steady-state conditions is controlled by the partitioning of chemical between the lipids of tissue and the organic carbon fraction of the sediment. The calculated BSAFs, should be used in lieu of station or species-specific values to "back out" tissue residue concentrations available for food chain transfer of organic contaminants. However, the Navy concurs that species-specific BAF data for metals should be used to calculate tissue residue concentrations for food chain assessments. Both BSAF and BAF data will provided in tabular format as Appendices in the revised report.

Specific Comment #5
Several errors and/or data omissions are evident in a variety of the tables:

 # Data in tables of the current report sometimes differ from those for the same COCs/samples presented in the May 1995 draft (e.g., current Table A1-1.6 versus May 1995 Table A-3.1.)

Response
Data validation and some analyses were not completed before issuing the May, 1995 document. Data changes were made as a result of validation.

 # The maximum reported sediment HQ (ER-L based) for copper in Table A.2-1.1 was cited as 5.33 whereas the maximum point-specific copper HQ in this table was listed as 5.38 for sample location AHW4. This suggests the tables were manually generated (a typo) and may have other internal data inconsistencies.

Response
The reviewer has misread the Table. The tables are not manually generated.

 # Table 3.3-1 has a different Phase II soils list of preliminary onshore COCs than that of Table 3.3-3 in the May 1995 draft; similar discrepancies among the lists of proposed offshore COCs appear among new Tables 3.3-1 and 3.3-5 and Table 3.3-3 of the May 1995 draft report. Also, why are the butyltin compounds chosen as offshore COCs in this new Table 3.3-5, without having first been identified as landfill-associated COCs in onshore soil and/or ground water?

Response
Discrepancies in the onshore and offshore list proposed COC list will be rectified.
TBT analyses were included at the request of the Ecorisk Advisory Board.

 # Table 3.3-2 incorrectly presents the ER-L for Total PCBs as 22.7 ug/g (ppm), when it should

be 0.0227 ug/g (ER-L is 22.7 ug/Kg or ppb.)

Response
The Table will be corrected.

 # Table 3.3-3 reports identical minimum and maximum values across four different classes of reference sampling locations for each of several metals (e.g., arsenic, cadmium, copper, mercury, nickel, and zinc) and aldrin, and among different VOAs within a location; are these actually detection limits?

Response
The reported values are ½ the Method Limit of Quantitation (MLQ).

Specific Comment #6
Table 3.3-4 incorrectly: (a) uses the simple sum of PCB congeners in the COC screening exercise, whereas total PCB concentrations should be calculated as (sum of congeners x 2); and (b) applied the reference concentrations of PCBs in the screening when the ER-L value of 22.7 ppb should have been used.

Response:
Table 3.3-4 will be corrected. Both reference and published criteria values will be applied to assess absolute and incremental risks as separate weights of evidence.

Specific Comment #7
Risk summary tables don't clearly show criteria-based, mean and maximum, COC-specific HQs and COC class-level HIs for each harbor subzone for the following:

 # Whole sediment (using ER-Ls and/or SQC)

 # Sediment pore water (using measured metals and EqP-inferred organic COC concentrations versus AWQC)

 # Surface water (using marine AWQC)

Response
Revised Risk Summary Tables will be provided as requested.

Specific Comment #8
No tissue data summaries, including minimum, mean, maximum, and location of maxima are provided for individual shellfish and fish species, that integrate both the RAPS (see report Table 3.1-1) and Phase III tissue data. (These empirical data are needed for use in the food chain models of the terrestrial ERA.)

Species & metal specific

Response:
The referenced tissue summaries were not provided in the main report because results of the food chain analyses (Section 6.3) clearly show that there are no statistically significant differences in BSAFs for organics among fish and shellfish species. Hence it is preferred to use the site-wide, COC class-specific BSAF values instead of individual target species statistics to calculate concentrations of organics in tissues as input for food chain models of the terrestrial ERA. For metals, a general similarity of BAFs among species was also noted, although a few species-COC pairings of notable exception were observed. Hence, species and metal-specific BAF values should be used to calculate concentrations of metals in tissues as input for food chain models of the terrestrial ERA. These empirical data will be added as Appendix tables in the revised report. RAPS tissue data were not included because of missing tissue lipid and sediment TOC data, as well as a lack of co-located sediment and tissue data.

Specific Comment #9
Corresponding summaries of COC/species-specific, BSAFs, which are not normalized for lipids or sediment total organic carbon (TOC) levels, should be provided based on tissue analyses of non-purged shellfish and whole fish (mummichog) samples. Although the lipid and TOC-normalized BSAFs presented are of scientific interest when comparing the BSAFs among species and/or locations, such BSAFs are not meaningful in assessing food chain exposures to the predators of these marine biota. Any use of molluscivorous and piscivorous food chain models by the Navy to calculate terrestrial risks to wildlife should incorporate non-normalized BSAFs (i.e., total tissue concentration:bulk sediment concentration) into the exposure assessment, based on data in this marine ERA, since shellfish/fish predators consume their prey whole, rather than extracting and devouring only the lipid fraction of their prey.

Response:
As discussed in Response to Comment #9, above, site-wide BSAF values should be used to calculate the whole body, non-normalized tissue concentration data for use in the food chain models of the terrestrial ERA, as the data show that there are no statistically significant differences in BSAFs for organics. The Navy concurs that food chain models used to calculate terrestrial risks of metals to wildlife should incorporate COC and species-specific BAFs (based on non-depurated data) since a predictive tissue:sediment equilibrium model analogous to EqP is not presently available for metals. The necessary data for terrestrial food chain calculations will be provided in the revised report.

Specific Comment #10
Additional discussions are needed in Section 4.3.1 to distinguish the mean and maximum, chemical-specific sediment HQs for specific polycyclic aromatic hydrocarbons (PAHs) versus HQs for Total PAHs (TPAH). The ER-L criterion for TPAH should be used to calculate HQs only for that subset of PAHs for which there are no chemical-specific ER-L values. The use of the TPAH value for all PAHs, otherwise, could underestimate the aggregate, PAH class-level risk (HI).

Response:
The text will be clarified to differentiate ER-L comparisons based on low molecular weight (LMW), high molecular weight (HMW) and total (tPAH) data summaries. The focus of discussion in Section 4.3 is to provide an overview of the magnitude of contamination at the site; specific discussions of Hazard Quotients for PAHs are provided during risk characterization (Section 6.5). Since the ER-L criterion for Total PAHs was developed based on the sum of all PAH analytes, this number should be applied only to tPAH values. The Navy concurs that inclusion of all analytes could result in overestimation of the aggregate, PAH class-level risk (HI), but this is a preferred alternative to misapplication of the ER-L value.

Specific Comment #11
Conclusions in the first paragraph on Page 6-6, that metals are probably not responsible for reduced sizes of the mummichogs at Station W5, since metal hyperaccumulation was not detected in the fish tissues at this location, contradicts earlier claims that fish tend not to bioaccumulate metals (e.g., 3rd paragraph on Page 3-22). Instead, natural age-related variation in fish size is offered as an alternative explanation for the smaller mummichogs at W5, since this explanation "...would be consistent with tissue metals data presented in Table 5.3-2, which do not indicate any obvious differences for Station W5 compared to the other stations." Since metals and/or organic compounds can be toxic to fish and impair their growth and reproduction even without being bioaccumulated, the natural variation argument, although theoretically possible, is not supported by the evidence as currently presented.

Response:

The data presented in Table 3.5-1 on fish morphometrics were used in correlation analyses with COC exposure concentrations and did not show dose-response relationship; length-weight relationships are average for the site population. Given a lack of dose-response relationships and also a lack of co-located exposure-effects data (i.e. nickel bioaccumulation at W4, reduced length at W5) it is difficult to conclude that reduced fish length at W5 is a true toxicological effect. Additional discussion will be added to the text to emphasize this uncertainty.

Specific Comment #12
The first paragraph in Section 6.2.2 on Page 6-6 seems to be missing some text, since its first sentence discusses regression analyses of mummichog tissue versus sediment metal levels, and the second sentence says that correlations were poor except for chromium in oysters. Is there some missing text on these fish and shellfish tissue/sediment metal regressions, that should appear between the two sentences of this paragraph?

Response
The text will be clarified to provide the missing information.

Specific Comment #13
Section 6.3, Analysis of Bioaccumulation, should, but does not, integrate RAPS data on COC

bioaccumulation data with those from this Phase III study. The COC "Biota-to-Sediment Accumulation Factors (BSAFs)." discussed here for fish and shellfish analyzed from Allen Harbor and the reference stations, should be combined from both studies and integrated into this ERA and the interdependent food chain models of the terrestrial ERA being prepared by EA.

Response:
RAPS tissue data were not included in the bioaccumulation because of missing tissue lipid and sediment TOC data, as well as a lack of co-located sediment and tissue data. The RAPs data will be reviewed to identify data appropriate for integration into this section.

Specific Comment #14
Section 6.3.2, Metals Bioaccumulation introduces the term "Biota Accumulation Factors (BAFs)," as "...representing tissue residues normalized to sediment concentrations," without distinguishing its meaning from that of the previously used term BSAF. This section also suffers from the misapplication of the HQ concept to calculate "BAF HQs," which are ratios of Allen Harbor:reference habitat BAFs. The discussions that follow about these "BAF HQs" seems not to be relevant to the purposes of the ERA, since incremental increases of COC bioavailability in Allen Harbor, above that for reference areas, is neither an assessment endpoint nor is needed for risk-based decisions about site remediation.

Response:
Additional text will be added to differentiate BSAFs,(being the lipid-normalized tissue concentration divided by total organic carbon normalized sediment concentration), from BAFs (tissue concentration divided by sediment concentration). The Navy concurs that the "BAF HQ" terminology is misapplied; additional text will be added to clarify that the BAF ratios are included to assess whether site specific conditions in Allen Harbor result in enhanced metal bioavailability (BAF ratio > 1), and thus could be more susceptible to risk from enhanced bioaccumulation.

Specific Comment #15
Section 6.5, Comparisons of COC Concentrations with Criteria and Standards, does not clearly convey a "big picture" of the overall magnitude and spatial patterns of ecological risks for different COC classes and exposure zones. For the most part, the text merely reiterates the content of data tables, such as the point-by-point locations of COC-specific exceedances of criteria, rather than discussing the average and maximum risks (HQs/HIs) from specific COCs and COC classes, calculated for the salt marsh, intertidal mud flat, and subtidal ecological zones.

Response:
The requested summary of overall magnitude and spatial patterns of ecological risks for different COC classes and exposure zones is provided in Section 7.2 of the report. As indicated in earlier comments, HQ summaries from specific COCs and COC classes, calculated for the salt marsh, intertidal mud flat, and subtidal ecological zones will be provided and included in Section 7

along with the zonation strategy as presently included. Further discussion of the criteria-based HQ data will be added to provide a better overview of risks as indicated by this weight of evidence, which will in turn support the zonation strategy outlined in Section 7.2.

Specific Comment #16
Please provide additional ecological rationale for the finely dissected subgroupings of sample locations into discrete ecological exposure subzones within Allen Harbor, as presented in Table 7.3-1 and Figure 7.2-1. Why was a more simplified grouping of data on sediment chemistry, biological effects, and medium-specific chemical risk quotients, into vegetated wetland (salt marsh), intertidal mud flat, and subtidal exposure zones, not used to demonstrate to the reader the evaluation of the weight of evidence gathered in the study? Also, the acronyms CP-VW and CP-SW (column headings) and CP-SD (legend) in Table 7.3-1 are not defined in the table and the table and figure should include all of the RAPS sample locations and analytical data for these zones.

Response:
Additional rationale for zonation will be provided in each of the risk characterization areas of section 6 to provide additional rational for the zonation strategy as presented in Section 7. Groupings based on simple classification by vegetated wetland (salt marsh), intertidal mud flat, and subtidal exposure zones, were not used initially since this could falsely suggest that the entire habitat groups present/not present ecological risks. This data would not be useful to risk managers, since only specific areas should be targeted for remediation. This point will be made in the text. Table 7.3-1 will be corrected.

Specific Comment #17
Zone-specific, quantitative risk quotients for each COC class (average HIs) should be incorporated into Table 7.3-1 or provided as a separate, similarly structured, synoptic table of results. These combined results should then be used to assess the statistical correlations, if any, among these qualitative and quantitative lines of evidence being used as ecological risk indicators for Allen Harbor, in hopes of clarifying which COCs may be responsible for the observed ecological effects.

Response:
The requested quantitative companion table to Table 7.3-1 will be prepared. These quantitative summaries will be discussed in the report, lending special emphasis as to which COCs may be responsible for the observed ecological effects.

Specific Comment #18
Additional evidence is needed to support the conclusion, presented in the second paragraph on Page 7-4, that "the sediments and shellfish tissues at intertidal and subtidal sites adjacent to Calf Pasture Point do not contain concentrations above those expected from regional input sources. In two sections of the executive summary a contrary conclusion was reached that "...significant or potential toxicity..." occurs "...at Calf Pasture Point." How does one reconcile this former

interpretation with the latter conclusion and with the observed toxicity of sediments to one or more of the test species at six of the ten locations sampled at Calf Pasture Point? Did the reference sediments exhibit similar levels of toxicity?

Response
The following text will be added on page 7-4 to the referenced conclusion:

> "The sediments and biota at vegetated, intertidal and subtidal sites adjacent to Calf Pasture Point do not contain concentrations above those expected from regional input sources; site:reference concentration ratios indicative of incremental risk were generally less than 10 for each habitat type and often had absolute concentrations lower than reference sites."

Additional text will be added to Section 6.6 to support this summary. The finding of significant toxicity in amphipods (Station W13) and sea urchins (Station D13) were not co-located, nor was there strong evidence of sediment risk drivers at either station (see Table 6.5-1). Other "potential" but not significant toxicity results were similarly not in spatial agreement with high HQ values. Hence the interpretation to be drawn from these results is that the weight of evidence does not support the conclusion of significant ecological risks occurring at Calf Pasture Point. The results can only be reconciled by concluding that there may exist COCs which are causing toxicity but these stressors are not severe or widespread in distribution. The text will be modified to clarify this uncertainty.

Specific Comment #19
The first complete sentence in the first paragraph on Page 7-7 refers to the missing Figure 7.2-3 and indicates that, for "TOC-normalized concentrations" of organics, "only 4 zones contained HQ values exceeding national criteria." Please clarify which national criteria are referenced in this statement, since TOC-normalized organic COC levels should not be used with ER-Ls to calculate HQs.

Response
TOC-normalized organic COC levels were compared against ER-Ls to calculate the HQs. The assumption of 1% TOC was made to add clarification as to the spatial distribution of risk in Allen Harbor.

Specific Comment #20
The meaning of the following should be clarified:

 # The statement in the penultimate paragraph on Page 7-10, that the investigation is "entirely synoptic."

Response
The text will be modified to indicate that sampling occurred over a limited temporal duration.

 # The last sentence of this same paragraph, which also is very confusing (As no prior findings of ecological risk were apparent in the prior study in concurrence with this study,...".)

Response:
The text will be modified to indicate that "The finding of similar chemical concentrations in sediments and lack of sediment toxicity between studies for areas of overlapping sampling (primarily in central Allen Harbor) suggests that seasonality or longer term temporal trends do not introduce a large degree of uncertainty into this assessment".

Specific Comment #21
The reference citation for Eisler (1993) is missing from the bibliography.

Response:
The reference will be added.

REVISED RESPONSES TO COMMENTS ON 25 AUGUST 1995 COMMENT RESPONSE SUBMISSION FOR UNRESOLVED ISSUES IDENTIFIED BY EPA IN 18 OCTOBER 1995 LETTER TO MR. PHIL OTIS FROM CHRISTINE WILLIAMS:

MARINE ECOLOGICAL RISK ASSESSMENT REPORT RESPONSE REVISIONS.

Comment on Response No. 9:
The clarification requested by the Navy is that the CoC data as presented in the report was not clearly linked, sample by sample, to discrete, explicitly defined/mapped ecological exposure zones (EEZs) as requested by EPA. Such EEZs should be distinguished from one another in terms of salinity regimes, channelized vs. non-channelized areas of wetlands, and other niche attributes that effect the foraging behavior by receptors. A clarifying look-up table is also needed , for all cumulative sample analysis data, that specifies from which of these EEZs data from each sample location was used to generate EEZ-specific summary data on CoC levels by medium (data for saline vs. freshwater areas should not just be broadly lumped together by watershed).

Response:
The Navy has presented in the report and at the November 1, 1995 meeting the ecological exposure zone strategy for Allen Harbor. Concurrence on this zonation strategy was reached as indicated in the meetings minutes. The Navy has agreed to develop a roll-up, quantitative summary table companion to the Weight of Evidence Summary Table (Table 7.3-1). Footnote data in that Table, which links specific station data to the EEZs, will be brought forward as a separate look-up Table. Text will added to the revised report to provide justification for the zonation strategy which will include the niche attributes of target receptors.

Comment:
Finally, the "number of HQ violations" should not be summed as proposed in the response, the actual, average of the HQs themselves should be summed, by CoC class for each discrete EEZ. (Point-specific sums by CoC class can be considered as an optional data presentation format.) See also the EPA comment letter dated October 12, 1995.

Response:
For the marine ERA, the actual, average of the HQs themselves were summed by CoC class for each discrete EEZ as presented in Figures 7.2-2 and 7.2-3. The number of observations (i.e. stations) comprising the sum was listed to provide an indication of the statistical confidence of the result (fewer stations averaged = less confidence). As mentioned in the comment response above, tabular data of these results will be presented in a companion table to Table 7.3-1.

Comment on Response No. 25.
Inadequate due to cross-linkage with Response No. 9. (Although this comment response applies largely to the offshore ERA for Allen Harbor (e.g., neoplasia), the requirement that all cumulative site contamination data be used in the ERA applies to the terrestrial ERA as well).

Response:
The requested data summaries will be provided as indicated in Comment Response No. 9, above. HQs for porewater and surface water will be developed following the strategy outlined in Response to General Comment No. 8, (this submission). Additional HQ correlations will be attempted in attempt to more fully identify key risk drivers. The Navy agrees to incorporate results of RAPs chemical data where possible. As indicated in the Nov. 1, 1995 meeting, these data are not readily available in electronic format hence are difficult to utilize. The Navy will make a best effort to do so, with documentation on difficulties and uncertainties associated with the combining of the two data sources.

Comment on Response No. 107.
The work plan originally had indicated that winter flounder was planned to be evaluated as a higher-level consumer until the Navy determined that they wouldn't sample the flounder.

The response however, did not clearly indicate how the Navy was going to assess the effects from the landfill on higher trophic level demersal fish. From the physical and biological data provided, it is likely that demersal fish (other than Fundulus) would use Allen Harbor and we believe this is a necessary assessment.

Response:
The work plan consistently indicated that winter flounder was not readily available in Allen Harbor, and that the mummichog was selected as a surrogate species for winter flounder. DEM has indicated that there are feeding habit differences between the two fish species which should be considered. The Navy agrees to evaluate this factor relative to trophic transfer and bioavailability of CoCs, and will accomplish this through comparison of BSAF values between

species (using literature data for winter flounder). Concurrence on this strategy was reached at the 1 November meeting (acknowledging that NOAA was not available for concurrence).

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